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Treatment versus regime effects of carrots and sticks

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Abstract:

Public Employment Service (PES) agencies and caseworkers (CW) often have substantial leeway in the design and implementation of active labor market policies (ALMP) for the unemployed, and they use policies to a varying extent. We estimate regime effects which capture how CW and PES affect

outcomes through different policy intensities. These operate potentially on all forward-looking job seekers regardless of actual treatment exposure. We consider regime effects for two sets of programs, supporting (“carrots”) and restricting (“sticks”) programs, and contrast regime and treatment effects on unemployment durations, employment, and post-unemployment earnings using register data that contain PES and caseworker identifiers for about 130,000 job spells. Regime effects are important: earnings are higher in a PES if carrot-type programs are used more intensively and stick-type programs are used less intensively. Actual treatment effects on earnings have a similar order of magnitude as regime effects and are positive for participation in carrot-type programs and negative for stick-type treatments. Regime effects are economically substantial. A modest increase in the intended usage of carrots and sticks reduces the total cost of an unemployed individual by up to 7.5%.

JEL Codes: J65, J68, J64

Keywords: active labor market programs, policy regime, treatment effect, employment, earnings, unemployment, caseworkers.

1 Introduction

Active labor market policies (ALMP) are important tools to fight unemployment and to improve the matching of workers and jobs in labor markets. Several OECD countries spend more than one percent of their GDP on ALMP. The existing literature has documented the effects of specific policy interventions on participants, see e.g. [Card *et al.* \(2010, 2017\)](#). But, interestingly, not much evidence can be found in the literature about the role of Public Employment Service (PES) units and caseworkers (CW) as policy makers. PES often apply mixtures of policies. Within PES, CW often have substantial leeway in dealing with their clients. Indeed, the frequency with which individuals are exposed to policies may vary substantially across PES and local labor markets.

This paper discusses the effects of PES and CW policy regimes on job seekers’ outcomes, notably on their job search durations and earnings. We

capture policy regimes as the intended use of a particular policy or program by a caseworker or PES unit that can not be explained by job seeker characteristics. Policy regimes affect job search strategies of potentially all job seekers. Job seekers who see that caseworkers tend to monitor strictly and tend to issue benefit sanctions frequently in the local public employment service may think twice about failing to send the required job applications. As a result of such a strict regime, potentially all job seekers search more intensely and/or have lower reservation wages. Training programs may exert a regime effect as well, although its sign is arguably ambiguous. Job seekers may find training attractive and reduce their job search intensity to improve the chances of attending training. Conversely, an intense training policy regime may provide a fertile environment for job seekers to know how to better find jobs. Determining the size and direction of policy regime effects is the object of our empirical investigation.

We estimate regime effects based on observed policy usage and on register data with PES and CW identifiers. We distinguish between programs with a supportive nature (“carrots”) and policies that constrain individual behavior (“sticks”).¹ The first group of policies is taken to cover training and job search assistance, and the second group to cover benefit sanctions and workfare programs. We observe how frequently the different PES and CW use these policies. To reconstruct intended policies from actual (observed) program participation or treatment exposure, we apply a competing risks approach that is ideally suited to our context where PES and CW determine ALMP assignment. The competing risks analysis enables us to measure ALMP exposures in a setting where the subsequent individual treatment status is not yet observed at entry into unemployment and where individuals can leave unemployment before being exposed at all. In a second step, we assess the relation between CW- and PES-specific intended policies and actual treatments on the one hand, and realized individual earnings and employment in the years after unemployment on the other hand. Thus, we examine medium to long run outcomes. The effects of these policies are assessed both on actual participants and on non-participants.

We jointly assess the relative importance of different kinds and types of ALMP effects. The effects of a “sticks” policy regime might depend on whether a “carrots” policy is present or not because job seekers are likely to face a second treatment after the first one. The presence of an intensive supporting carrots regime might lead job seekers to feel that the sticks policy is less of a threat, so that the threat effect of sticks policies may be lower if used in combination with carrots policies. Carrots policy regimes, on the other hand, might need to go one-for-one with sticks policy regimes. For instance, a PES that places many job seekers into training might want to enforce a rigorous adherence with job search requirements. Our empirical strategy allows us to directly identify the contributions of specific policy makers (PES and CW). Note that ignoring the effects and interplays of policy regimes means ignoring a potentially non-negligible part of the over-all ALMP effects – and thus means ignoring the role of the policy makers and policy implementers in UI systems.

In our empirical analysis we use a rich base of register data from Switzerland. Switzerland provides an especially useful setting for analyzing the role of policy regimes. The PES enjoy a large leeway to forge their specific strategy in implementing the different types of policy (i.e., including what we refer to as carrots and sticks). As a rule, job seekers are assigned to CW based on exogenous and pre-determined characteristics (last name, industry, etc.). Conditional on these characteristics, assignment to CW is plausibly random. For PES policies, we analyze outcomes within labor market regions. The latter have been created originally to cover travel-to-work areas and represent local labor markets. As our baseline sample, we take a fourth of the complete inflow of men into registered (full-time) unemployment in Switzerland in the years 2000 to 2005, up to age 61.² This covers over 150 different PES and 700 CW. The unemployment insurance database provides a large amount of socio-demographic and benefit-entitlement-related information. To this base we merged a further database that covers the (daily) history of all ALMP events, including sanctions. Finally, to observe the outcome and the past employment history, we added social security data (monthly precision) which

covers (non-)employment and earnings in the six years before and up to 42 months after unemployment entry.

Policy regime effects can be seen as a generalization of the concept of ex ante effects of possible future treatments on not-yet-treated individuals. Ex ante effects are generated by individuals being concerned about future treatments. When deciding about their behavior before a treatment arises, it is optimal for them to take into account that there is a rate at which they will be exposed to a treatment and its subsequent effects. Regime effects do not only capture ex ante effects before a first treatment, but they may also apply after a treatment has occurred, in anticipation of subsequent treatments and interactions with the PES or CW. Indeed, they may capture a general comprehensive guidance approach of the PES and CW towards their clients, over and beyond the assignment of treatments. For example, higher intensities of support or constraint may stretch beyond treatment assignment, towards a high degree of helpfulness or towards a highly controlling attitude, and each of these may be reciprocated in the job seeker's behavior. Regime effects may also include information spillover effects regarding policy intentions. Policy regimes as well as ex ante effects are related to equilibrium effects through changes in labor demand and labor supply and their composition.³ However, at the CW level (that is, when capturing CW effects in deviation of the corresponding PES level), no equilibrium effects should be expected, and it is even highly questionable whether PES areas are sufficiently large to induce such effects.

Our empirical analysis does not provide final answers on what are driving forces behind the estimated regime effects (although we use external data sources to rule out that caseworker personality and the PES corporate style are critical). In this sense it is interesting to briefly examine findings in the existing empirical literature on ex ante effects. For job search assistance programs, these are analyzed in, for example, Blundell *et al.* (2004) and van den Berg *et al.* (2015). For training programs they are analyzed in van den Berg *et al.* (2009). The former studies exploit the national introduction of a new policy whereas the latter study uses self-reported assessments by

newly unemployed workers about the rate at which future treatments take place. Ex ante effects of sticks policies have been analyzed in studies of policies in which the compliance to job search directives for unemployment benefits recipients is monitored. A relevant study is Rosholm and Svarer (2008) who examine ex ante threat effects of activation policies by allowing the transition rate to work to depend on the transition rate to ALMPs, which are simultaneously estimated. We discuss their study in more detail in Section 4.⁴ The studies generally find evidence of what may be called ex ante attraction: individuals who expect participation in a supporting program may reduce search intensity before the treatment and become more selective in terms of the jobs they accept, whereas for constraining programs this is reversed. Of course, even as a study of ex ante effects, our study goes beyond this literature in that we simultaneously consider different treatment types and their interaction effects.

A few studies in the literature on the treatment effects of ALMPs jointly estimate effects of different treatments and their interactions. In this subset of the literature, even fewer consider a contrast between supporting and restricting programs. van der Klaauw and van Ours (2013) is an exception, studying the effect of both re-employment bonuses and benefit sanctions on the re-employment chances of welfare recipients. Also, van den Berg *et al.* (2010) show that newly unemployed workers report widely different subjective probabilities of future participation in training programs and in workfare, and that this is reflected in their job search behavior.⁵

Yet another related branch of literature studies CW-driven effects on unemployed individuals' outcomes. These effects, captured by activities like counseling or monitoring, appear to be substantial. For a recent overview of the evidence, see Rosholm (2014). This literature, however, does not directly assess the contribution of the CW to the estimated treatment effects (due to missing CW identifiers). In addition to this, there is evidence that CW do use their discretionary power, in that the variation in CW-induced ALMP assignments is substantial across caseworkers after correction for worker characteristics (see Eriksson (1997) for an early randomized study). There is

an analogy to the effect of physician-specific effects on sickness absence; see Markussen and Røgeberg (2013). Huber *et al.* (2017) use mediation analysis to study the roles of various facets of caseworker personality in the evaluation of labor market outcomes.⁶

The next section provides information on the institutional background of the empirical analysis, in Switzerland during our observation window. Section 3 presents the data and provides a descriptive analysis.⁷ Section 4 presents the empirical approach to estimating policy regimes and discusses identification of the main parameters. We pay particular attention to the issue that individuals may influence the (latent) rate at which certain “sticks” treatments arrive. We also examine whether a relation exists between caseworker policy regimes on the one hand and the personality of the caseworker in his behavior towards clients on the other hand, since in the current study we are interested in the former but not in the latter. Here, as in other parts of the paper, we exploit insights from in-depth survey interviews held among caseworkers and PES offices. Section 5 provides a descriptive analysis of the measured policy regimes. Section 6 presents the main results. Here we also study various interaction effects between policies, and we provide a comprehensive cost-benefits analysis. Section 7 concludes.

2 Institutional Background

The entitlement duration of unemployment insurance (UI) benefits in Switzerland is 400 days for individuals who meet the contribution and employability requirements. From age 55 onwards, benefits are extended by an additional 120 days. The replacement ratio is 80%; however, it is 70 % for those who earned more than CHF 4030 per month prior to unemployment and who are not caring for children.⁸ Job seekers have to pay all earnings and social insurance taxes except the UI tax rate (which stands at about 2%). This means that the gross replacement rate is close to the net replacement rate. After the entitlement period, the unemployed have to rely on social assistance. The latter is means-tested and equals about 76% of

unemployment benefits for an individual who is single and has no other sources of earnings.

Enrollment in UI has two requirements. First, the individual must have paid UI taxes for at least twelve months in the two years prior to registering at the public employment service (PES). Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job - they must be “employable”. If a job seeker is found not to be employable there is the possibility to collect social assistance.

The entitlement criteria during the unemployment spell concern job search requirements and participation in active labor market programs. Job seekers are obliged to make a minimum number of applications to “suitable” jobs each month⁹ and they are obliged to participate in active labor market programs during the unemployment spell.¹⁰ Compliance with the job search and program participation requirements is monitored by roughly 2500 caseworkers at 150 PES offices. When individuals register at the PES office, they are assigned to a caseworker on the basis of either previous industry, previous occupation, place of residence, alphabetically or the caseworker’s availability. Job seekers have to meet at least once a month with the caseworker. Caseworkers monitor job search by checking that job seekers fill in the details of the jobs to which they have applied (monthly protocol of applications) and by asking them to present the sent applications at the meetings. Job seekers are typically required to apply to about 8 to 10 jobs per month. Participation in a labor market program is monitored by the caseworker because program suppliers only get paid for the actual number of days a job seeker attends the program. Moreover, non-participation is subject to sanctions as well (Lalive et al., 2005; Arni et al., 2013).

There is *remarkable discretion* in how often labor market programs and sanctions are used across PES. The authorities at the level of the canton and, in particular, the caseworkers have considerable leeway in the strictness with

which rules are followed and guidelines are applied. With respect to sanctions, caseworkers may adjust, to some degree, the target number of required applications and the monitoring intensity. Caseworkers count the number of new applications in all cases and they may also check up on the applications claimed by job seekers. In the case of labor market programs, caseworkers dispose of some discretion in the assignment decision, with respect to participation, choice of program type and timing (Behncke et al., 2010a).

The Swiss labor market policy distinguishes between four types of policy treatments: (i) Human capital training programs (this includes, as the mostly used sub-category, job search assistance programs); (ii) workfare programs (within public or non-profit institutions); (iii) subsidized temporary employment (during the unemployment spell); (iv) sanctions.

In this paper, we regroup these into two distinctive program types: *carrots* and *sticks*. The first group, *supporting* programs, comprise all kinds of training and job search assistance, thus type (i). The second group, *restricting* programs, aggregates sanctions and workfare programs, thus types (ii) and (iv). The reason why we consider workfare programs first and foremost as sticks is that they are broadly disliked by the job seekers. Thus, they try to avoid them – for reasons of stigmatization and fear to be “locked in” into these programs over the longer period – by not proposing them to caseworkers. The above-mentioned survey by Behncke et al. (2010a) provides evidence that supports this interpretation. Restricting programs are mainly sanctions (80% on average), so effects of restricting programs are most likely generated by their sanction component, not the workfare component. We do not explicitly model subsidized temporary employment, treatment (iii), because job seekers choose the subsidized jobs by themselves, so caseworkers do not have much discretionary choice in this respect. Also, job seekers with subsidized employment remain eligible for carrot and stick programs.¹¹

3 Data and Descriptive Statistics

Our analysis uses data from two sources. The unemployment insurance register contains administrative information on all spells of registered joblessness. For our sample we extract all the spells that started between July 2000 and June 2005 for job seekers who were 61.5 years old or less when they registered at the PES. This data records unemployment duration: this is the number of days a job seeker is registered with the local PES. Note that unemployment duration can deviate from days on unemployment benefits. Individuals may register with the PES before they lost their job. Job seekers may, in principle, also de-register before they start on the new job. Unemployment duration is still a useful concept for our analysis since job seekers need to be registered to follow ALMPs. The data also contain detailed information on the timing of ALMP participation and benefit sanctions events in daily precision. The data informs on where job seekers live, which PES is in charge of the job seeker, and also information on the caseworker in charge. Usually, caseworker assignments are fix over the course of the unemployment spell but there are exceptions¹². We focus on the caseworker initially assigned to the individual. We have detailed information on socio-demographics, employability, occupation, benefit variables, household size, and whether a person has filed an application for disability insurance benefits.

Our second data source is social security register data. This data covers a 25 % random sample of all workers between 1982 to 2008. The data provide information on employment and earnings for every month between 1982 and 2008. We use this data source to construct 5 years of pre-unemployment history for every spell of joblessness. We also use it to construct our main outcome variables. We look at real monthly employment earnings in the period of 3.5 years after leaving unemployment. We also separately record the number of months a job seeker has been employed, and the average earnings during the employment months during the 3.5 year post-unemployment period. This allows us to decompose earnings into an employment and into earnings while employed component.

Table 1 provides descriptive statistics of the key variables for our main estimation sample of 131,037 job search spells of eligible men aged 20 to

61.5 years (91,705 individuals).¹³ In our analysis, we focus on the first treatment that job seekers receive during their unemployment spell. About 22 % of all job seekers enter a supporting program, and 19% of all job seekers are receive a restricting treatment. The median time until the supporting program starts is 97 days. Restricting treatments begin somewhat earlier, after 71 days.¹⁴ Most job seekers are either married (46 %) or single (45 %), and fewer ones are divorced or widowed (proportion not shown). A substantial proportion of job seekers in our data have completed a 4 year vocational training after compulsory schooling (50%). The second most important educational attainment is compulsory schooling (28%). Relevant proportions of job seekers have either completed a short vocational training of 2 years (9%) or a tertiary degree (10 %). Male job seekers typically work as blue collar factory workers (13%), construction workers (13 %), or in the restaurants or cleaning sector (13 %). Descriptive statistics also show information on employability, a caseworker assessment of the chances the job seeker will find work. Most job seekers have medium employability indicating no large problems with job placement (72%), but a sizeable proportion also have low employability (15 %) as well as excellent employability (13 %, not shown in table). Job seekers are 36 years old on average, on average living with 2.16 persons in a household. About 42 % of all job seekers do not have Swiss nationality, and 39 % do not speak the local language as their mother tongue.

The median unemployment duration in our sample is 144 days. We measure unemployment as the number of days between registering at the PES agency until de-registering from the PES. This is the period during which job seekers in Switzerland have access to active labor market programs (regardless of their current employment status).

Figure 1A shows the empirical exit rate from unemployment, i.e. de-registrations from the PES. Job seekers leave unemployment initially at a rate of 10 percent per month. The transition rate then increases, peaks at 15 percent per month after 3 months of unemployment, and gradually decreases to 7 percent per month after 18 months of unemployment. Benefits end for most job seekers after 18 months of unemployment. As usual, we observe an

increase in the transition rate out of unemployment shortly before the expiration of benefits entitlements.

Figure 1B shows the empirical transition rate from unemployment to a supporting program. In the beginning of the unemployment spell, just short of 4 percent of all job seekers start a supporting program. The probability of entering a supporting program then increases to a maximum of 7 percent per month, and it decreases gradually to a level just above 1 percent after 22 months. The transition rate to restricting programs follows a fairly similar pattern, but it is substantially below that of supporting programs throughout the unemployment spell. Note that the duration dependence of all transition rates that we show in Figure 1 might be spurious as we do not control for heterogeneity in these plots.

We now turn to discussing employment and earnings measures. Figure 2 shows earnings and employment paths for the job seekers in our estimation sample, relative to the calendar date of PES registration, which is normalized to zero on the horizontal axis. “Earnings for employed workers” represent average earnings among individuals who are employed during a month. Employment is the proportion of individuals in our sample who hold a job in a month. These two measures can be combined into our total average-population “earnings” measure. In employment, these “earnings” are taken to equal to actual earnings whereas in non-employment they are set to zero. The total “earnings” measure can decrease for a number of reasons: either employed workers are paid less, or fewer individuals hold a job, or both.

Total average “earnings” increase somewhat before the unemployment and decrease sharply upon entering unemployment.¹⁵ Note that they do not reduce to zero. There are two reasons for this. First, a substantial proportion of job seekers register at the PES even before losing their job. Secondly, very short unemployment durations may lead to non-zero earnings in the months on or right after the unemployment registration. By construction, the average “earnings while employed” exceed the average total “earnings”.

Unemployment does not reduce earnings compared to the pre-unemployment level.

Figure 2B shows employment. Most job seekers are employed before registering at the PES, even though the employment rate is far from 100 % in the month prior registering. The employment rate then decreases substantially but does not reach zero in the month when job seekers register at the PES. Again, this shows that entering our state of “PES-registration” unemployment and leaving a job are not necessarily concurrent. The employment rate increases rapidly over the first 10 months after the onset of the spell.

4 Conceptual Framework

In this section we explain the methodology and we show how its key assumptions are justified by the institutional setting of the Swiss labor market.

4.1 Variation in policy regimes

We capture policy regimes as the intended intensity of use of a program. Policy regimes therefore refer to the speed or frequency at which a decision maker aims to use a particular policy instrument, e.g. training programs, over and above the frequency of use indicated by the characteristics of the job seeker. In Switzerland, policy regimes may vary across PES offices and across caseworkers. They may vary at the PES level because this is the de facto unit that implements the procedures leading to policy regimes. A 2003 survey among 98 heads of PES shows to what extent PES directors are managed by the canton, and how strictly they manage their caseworkers (Table 2). Of course, heads of PES are not completely free in their work, and most of them do not let their caseworkers do as they please. However, more than half of the heads of PES only receive rough guidelines and are free to define their strategies within those guidelines. A similar proportion sets only rough guidelines for caseworkers and lets them choose freely within those guidelines. Our empirical approach will exploit exactly this within-canton and within-PES leeway to estimate policy regime effects.

4.2 Quantifying different policy regimes

Actual usage of the program does not necessarily provide an accurate description of intended usage, for two reasons. First of all, an individual may leave unemployment before participating in the program. Secondly, regimes may include ex ante effects which, depending on the policy, may allow individuals to influence the actual usage of the treatment themselves. We now describe in turn how we deal with these issues.

Competing risks.

The first issue can be dealt with by invoking duration analysis with competing risks. Let t_u , t_c and t_s denote the time (in unemployment) until de-registering from the PES, participation in a carrot ALMP, and exposure to a sticks policy treatment, respectively. Next, let t_p denote the duration until the first event in the unemployment spell. The latter follows a competing-risks process:

$t_p = \min(t_s, t_c, t_u)$. We define the corresponding treatment dummies as follows: $D_c = 1$ if $t_p = t_c$, and $D_s = 1$ if $t_p = t_s$.

Our operationalization of the intended policies by PES and by CW is based on the hazard rates of the latent durations t_c and t_s , respectively, for each PES and for each CW, adjusted for individual characteristics x and for the elapsed unemployment durations at the moment of treatment. For $p = s, c$ we adopt the following proportional-hazard functional forms,

$$\begin{aligned} \theta_p(t, x, D^{PES}, D^{CW}, Y^-, \tau, m) = & \lambda_p(t) \exp(x' \beta_p + \sum_{j=1}^{N_{PES}} \gamma_{p,j}^{PES} D_j^{PES} \\ & + \sum_{k=1}^{N_{CW}} \delta_{p,k}^{CW} D_k^{CW} + (\alpha_p)' Y^- + \sum \mu_{p,m} + \sum \eta_{p,\tau}) \end{aligned} \quad (1)$$

Here, t is the elapsed duration of unemployment. PES is the public employment service, and D_j^{PES} are a full set of dummies that identify the PES for each individual. CW is the caseworker, and D_k^{CW} refers to a full set of dummies that identify caseworkers. The η coefficients constitute a set of half-yearly inflow cohort fixed effects (where τ denotes calendar time at the moment of inflow), Y^- is a series of variables that controls for the earnings history of the individual in the last 60 months before unemployment (split in 17

time intervals/parameters), and μ constitutes a set of labor-market-region fixed effects (where m denotes the region). To demarcate units for these spatial fixed effects, we use the 106 commuting zones constructed by the Swiss authorities to capture local labor markets. These zones are usually called *Mobilité Spatiale* regions (in short, MS regions). These do not coincide with PES. In our data, there are on average 4.6 PES per MS, with a standard deviation of 2.9. In cities constituting one MS there may be a considerable number of PES. For example, the metropolitan areas of Geneva and Zürich contain 11 and 8 PES, respectively. Notice that if PES regime effects are assumed absent, we may use the PES as the relevant spatial unit for the spatial fixed effect.

The model (1) assumes that policies are stable in time. We have probed the stability of regime effects by ranking caseworkers and PES units with respect to the rate at which they use carrots and sticks. We find that PES and caseworkers use the policy tools in ways that places many of them at a similar rank from one year to the next.¹⁶ So policy regimes appear, on the whole, stable over the time horizon we analyze.

Before estimation, we normalize caseworker effects by defining them in deviation from the PES mean effect. This means that the PES effect captures the PES policy regime including the average effects of its caseworkers. Caseworker mobility across PES would enable the separate identification of the PES regime effect and the average effect of the composition of the PES team of caseworkers. However, such mobility is sparse in our observation window, and we would have to restrict the analysis to a tiny fraction of the set of PES. We therefore proceed by not imposing the constraint that the caseworker effects of caseworkers moving between PES is identical across PES.¹⁷ We also transform the PES effects as deviations from the mean of the MS region and MS effects as deviations from the economy-wide mean. The latter ensures that the intercept in each model reflects the population average outcome.

In the absence of systematic unobserved heterogeneity, the above hazard rates θ_c and θ_s can be estimated in isolation from each other. Specifically, the competing-risks approach treats all t_p spells that end in an event that differs from participation in the program of interest as independent right-censoring of the time to participation in that program.^{18, 19} Notice that this is not related to the existence of a policy (regime) effect of θ_c and θ_s on t_u .

Next, for each individual with a given x and a given PES and CW, we can estimate individual probabilities that a treatment event occurs within two years in the absence of other events, using²⁰

$$F_c^L = 1 - \exp\left(-\int_0^{730} \theta_c(t, x, \{D_j^L\}, Y^-, \tau) dt\right) \quad L = PES, CW$$

and analogously for F_s , with θ_c and θ_s specified as in equations (1). As explained by e.g. Van den Berg (2001), the signs and relative magnitudes of the estimated covariate effects on (one minus) the survival probability are robust with respect to the omission of unobserved heterogeneity, whereas this does not always apply to the covariate effects on the hazard rates. This is one reason to prefer F_c and F_s over θ_c and θ_s as regime indicators. A second reason is that F_c and F_s naturally cover a time interval whereas θ_c and θ_s assume different values at different elapsed durations.

Influencing the treatment rates.

We now turn to the second issue mentioned at the beginning of this subsection, namely that job seekers may ex ante influence the rate at which they are treated in response to the perceived policy regime. In that case the competing-risks hazard rates θ_c and θ_s depend both on the regime and on the reaction to the regime, so that they may not fully characterize the intended policy intensity anymore. With supporting (carrot) policies this is not likely. If the corresponding treatments are deemed attractive then their supply will always be rationed by the administrative unit. Even if individuals can influence the rate at which they participate in a carrots program, the ranking of the estimated latent hazard θ_c across PES (or CW) will probably not revert the ranking of intended usage across PES (or CW).²¹ Therefore, the indicators

based on this hazard should still reflect the ranking of intended usage. As a result, the effects of the indicators we constructed are still informative on the effects of the intended usage.

With restricting (sticks) policies such as workfare, a similar line of reasoning can be applied. However, with sanctions, the individuals have a much stronger influence on the occurrence of the treatment. As a result, the effect of the strictness of the policy regime on the sanction rate (i.e., on the rate that is estimated in the competing risks analysis) may be non-monotonic. To see this, consider a policy where individuals' search effort s is stipulated to meet or exceed a lower threshold value s^* . Individuals suspected to violate this rule are monitored at the rate p_0 , and if it is detected that $s < s^*$ then a sanction is imposed.²² The strictness of the policy regime is then p_0 whereas the sanction rate is $p_0 \cdot \mathbb{I}(s < s^*)$. The former is the quantity of interest whereas the latter is obtained from the competing risks analysis (it equals θ_s in the absence of other sticks policies). Clearly, if $p_0 = 0$ then both of these are equal to zero. If p_0 increases then the fraction of individuals with $s < s^*$ will decrease because of the strategic ex ante reaction, but the actual sanction rate will then typically be positive. However, if $p_0 \rightarrow \infty$ then each violation leads to a punishment. If the punishment is sufficiently large then each individual will choose s such that $s \geq s^*$. Thus, the policy-regime strictness goes to infinity but the sanction rate goes to zero. As a result, an estimated sanction rate of zero is compatible both with a very lax regime and with a very strict regime. This means that the estimate of θ_s is not informative of the intended policy, unless we restrict attention to low to moderate levels of the intended usage and/or sanctions only constitute a minor fraction of the total package of sticks policies. These conditions are met in our setting.

4.3 Effects on outcomes

Outcome equations.

We are interested in measuring how policy regimes affect the unemployment exit hazard and, in particular, the earnings after leaving unemployment.

Concerning the former we estimate the following specification for the unemployment exit hazard

$$\theta_u = \lambda_u(t) \exp(x' \beta_u + \delta_{s,u} D_s(t) + \delta_{c,u} D_c(t) + \pi_{s,u}^{cw} F_s^{CW} + \pi_{c,u}^{cw} F_c^{CW} + \pi_{s,u}^{pes} F_s^{PES} + \pi_{c,u}^{pes} F_c^{PES} + (\alpha_u)' Y^- + \sum \eta_{u,\tau} + \sum \mu_{u,m})$$

whereby the notation is as in the previous subsection. Note that we exploit past earnings information to control for employment-related selective differences between the individuals. Also note that the treatment dummies D are now time-varying, to distinguish between the time before and after treatment. In subsequent analysis we also allow for interaction effects between the various policy regime indicators.²³

Similar to the above equation, the effects of policy regimes and treatments on earnings and employment after the unemployment spell ended are modelled as follows:

$$Y = x' \beta_Y + \delta_{s,Y} D_s + \delta_{c,Y} D_c + \pi_{s,Y}^{cw} F_s^{CW} + \pi_{c,Y}^{cw} F_c^{CW} + \pi_{s,Y}^{pes} F_s^{PES} + \pi_{c,Y}^{pes} F_c^{PES} + \tau' f(t_u) + \alpha' Y^- + \sum \eta_\tau + \sum \mu_m + \varepsilon_Y$$

Y can represent different post-unemployment outcomes (over a time window of 3.5 years), such as average monthly earnings or employment probabilities (i.e. employment stability measures). The above equation is the key outcome equation. We control for the completed²⁴ unemployment duration t_u in polynomial form to isolate the effect of regimes on earnings that arises directly, i.e. without changing unemployment duration. We include past earnings to address the “pre-program earnings dip” (Ashenfelter, 1978) pattern in earnings, i.e. job seekers’ earnings rapidly deteriorate before entering unemployment, but recover after starting the unemployment spell. Also note that, jointly, the analyses of employment and of earnings while employed entail a decomposition of the total effect on post-unemployment earnings.

Discussion of identification.

Since the equations to be estimated are regression-like specifications without complex error structures, all model parameters are identified provided that regressors are not perfectly correlated to each other. However, it is interesting to discuss the key assumptions that underlie the identification of the causal effects of interest from the regression specifications.²⁵

First, notice that to identify the causal effects of attending a particular program and the causal regime effects, we make conditional independence assumptions (CIA). To this purpose it is important to point out that our data contain a wide range of individual summary measures of earnings histories and covariates, notably those that are seen as forming the information set of the institutions deciding on treatment plans (education, age, past occupation, function and unemployment, language skills, benefit conditions, etc.)²⁶. In this sense our approach follows the large range of evaluation studies with Swiss labor market data (see the references in the introduction).²⁷ The outcome equations include treatment and regime effects within the same equation, so regime effects are identified conditional on treatments and vice versa. Obviously, this means that inference on regime effects and treatment effects requires less assumptions than in the case where only treatment effects or only regime effects are analyzed. For example, our treatment effect estimates control for the fact that the caseworker may influence both the individual treatment status and the outcome of interest, where the latter channel runs through the policy regime imposed by the caseworker.

To further justify the CIA underlying the identification of causal caseworker regime effects we examine how job seekers are allocated to caseworkers. The Behncke *et al.* (2010a) survey provides information on this (multiple answers are possible): 24 % of all caseworkers indicate that their clients are assigned randomly, 50% by industry, 55% by occupation, 44% by workload.²⁸ Hence, random or quasi-random assignment of regimes appears plausible.

There is a thin line between the CW policy regime and what we might call the “caseworker style”. Caseworkers may differ in terms of their personality and how this impacts on their interaction with clients: how friendly they are, how

much empathy they feel for them etc. Such a caseworker style may be correlated with the caseworker's ALMP assignment policy. The interpretation of our regime effect estimates depends on this. If caseworker style is important and correlated to our regime indicators then the estimates at least partly reflect how the CW interact with job seekers on a daily basis. Behncke et al. (2010b) show that caseworkers differ in their attitudes to their work: some see it as their prime task to help their client, some focus on controlling their clients, and not all think that all this matters for job search success. We further assess this issue using the Behncke et al. (2010a) survey. We first construct a measure of how important each caseworker believes restricting or supporting policies are.²⁹ We then correlate the importance of supporting and restricting programs with caseworker style. We find absolutely no correlation with supporting policies (correlation coefficient -0.0234) and a small, positive, correlation with restricting policies (correlation 0.1345). Thus, we feel confident that caseworker style does not drive our results.³⁰

Identification of PES regime effects requires a CIA (within MS region) of PES regime with respect to outcomes. This assumption appears plausible as job seekers can not choose which PES takes care of them, so endogenous mobility between PES is not an issue. We can not rule out that "PES style" plays a role. But our data cover a very wide range of activities that feed into the job search process. We are likely to capture most of these activities. Moreover, we discuss below that the two key activities of a PES, assignment to restricting programs and assignment to supporting programs, are virtually unrelated. Orthogonality between these two key policy dimensions bolsters our confidence in our assumption of orthogonality with respect to other unmeasured dimensions.

Identifying treatment effects further requires the assumption of "no anticipation" (NA) (Abbring and van den Berg, 2003). In words, potential outcomes should not depend on the moment at which future treatments are realized, any more than what is captured in the model specification. Notice that NA does not preclude regime effects. It rules out that individuals have and use more advance knowledge on the timing of future treatments than what is captured

in the model specification. Precisely because we allow for heterogeneous regime effects, the NA assumption is less restrictive in our setting than is usually the case. After all, the actual regime is likely to predict the speed at which a treatment takes place. NA is justified for Swiss ALMP, as the time between the knowledge that a decision is made to assign a program or a sanction, and its realization, is usually shorter than two weeks (Lalive et al., 2005, 2008).

We finish this subsection by comparing our methodology to Rosholm and Svarer (2008) who developed an innovative approach to estimate ex ante threat effects of ALMP. They restrict attention to activation policies. Specifically, they estimate a multivariate duration model, for the duration until an activation program and the unemployment duration, controlling for selection on unobservables by way of a random effects specification. In addition, they include the transition rate to ALMPs as an explanatory variable for exit out of unemployment, in order to capture the ex ante threat effect of activation programs on the exit out of unemployment. This resembles the role of the “carrots” regime indicator F_c as a regressor in θ_u . Identification relies on the requirement that the covariates in (a function of) θ_c and the other covariates in θ_u do not act additively in θ_u . This contrasts to our approach in which we exploit caseworker and PES identifiers to characterize the policy regime.

5 Descriptive Analysis of Policy Regimes

To gauge the variation in the actual usage of the “carrots” and “sticks” policies and the variation in estimated policy regime intensities, as well as their interrelations, this section provides some descriptive statistics. The observed frequency of usage of a policy (or the “observed intensity”) is measured by the frequency of imposition of at least one treatment of the respective program type within a spell of unemployment. On average we observe that one in every five individuals (0.22) is subject to a training or job search assistance program and also that one in every five individuals (0.19) is sanctioned or has

to join a workfare program during unemployment. This is true both for PES and for caseworker regimes (Table 3).

Not surprisingly, the policy regime intensities (or “intended policy intensities”) as estimated in Subsection 4.2 are substantially higher. About three job seekers out of five (0.58) would enter a supporting program within two years if there is no possibility to leave unemployment or to be confronted with a restricting program. Likewise, about one in two job seekers (0.53) would face a restricting program according to the intended policy regime. The standard deviation of the intended policy intensity is also substantially larger than the standard deviation of the observed intensity.

Figure 3 plots intended vs observed intensities across PES.³¹ Intended policy intensities are always larger than observed policy intensities. If this were not the case for some PES then this would signify a model specification problem in the sense that the specifications of θ_s or θ_c are too restrictive. The discrepancy between observed and intended intensities tends to be especially important for extreme regimes, i.e. those that intend to place everyone to a supporting or to a restricting treatment.

The fact that actual and intended policy usage are not perfectly related is important for at least two reasons. First, it means that discarding the competing risks analysis and instead using actual observed intensities would lead to biased effects. Secondly, since PES equilibrium effects are captured by actual usage by PES rather than intended usage by PES, it follows that our intended PES policy regime intensities are not synonymous to PES equilibrium effects.

We are also interested in the degree of concurrence of restricting and supporting policies. Figure 4A reports the variation of policy mixes, i.e. combinations of carrots and sticks policy intensities. The actual observed policy mixes broadly cover the two-dimensional policy space in the ranges between 0 and 0.4. This suggests that there is substantial two-dimensional

variation to support its exploitation in our estimation strategy. Figure 4B shows the corresponding results for policies at the caseworker level.

Figure 4 also displays the absence of a correlation between carrot and stick policy regime intensities. This could mean that PES regime for one policy is determined in isolation of the regime for the other policy. Somewhat speculatively, one might say that this is consistent with the maintained hypothesis that “PES style” is not driving our results. After all, if PES were planning their comprehensive policy regime mix it seems plausible to observe a correlation between the intensities of the regimes.

6 Results

6.1 Baseline estimation results

Effects on earnings.

Table 4 reports results on earnings. The dependent variable captures average earnings after leaving unemployment over a period of 42 months (3.5 years).³² All estimates control for the full set of individual control variables and a full set of PES dummies (columns 1 to 5), or labor market region (MS) dummies (columns 6 and 7).

Column (1) to (4) in Table 4 show the effects of program participation. Supporting treatments increase earnings; restricting treatments decrease them. Sanctions are especially detrimental to earnings after leaving unemployment, reducing them by 348 CHF or about 10 percent of average monthly earnings (Column 4). Workfare programs also reduce earnings but the reduction is 70 CHF per month, around 2 percent of monthly earnings. Estimating the program participation effects jointly reveals that the treatment effect of carrots is somewhat smaller, and the sticks effect is not as negative, since the baseline earnings now is non-participants for both estimates. Attending a supporting program increases earnings by 153 CHF per month, around 5 percent of earnings. A restricting program reduces earnings by 309 CHF or almost 10 percent.

Columns (5) to (7) in Table 4 discuss policy regime effects, on top of program participation effects. Caseworkers who intend to use restricting programs more frequently reduce their job seekers earnings after leaving unemployment, and the effect is sizeable. Increasing the intended use of restricting programs by 10 percentage points reduces a job seekers earnings by 51 CHF, or around 1.7 percent. Caseworkers who use supporting programs more frequently do not affect their job seekers' earnings.

Column (6) shows that the variation in policy regimes at the PES level also matters. Increased use of restricting policies reduces job seeker's earnings. The PES effect is stronger than the caseworker effect: a 10 percentage point increase in the use of restricting programs decreases a job seeker's earnings by 106 CHF per month, about 3.4 percent of earnings. Interestingly, increased use of supporting programs has the opposite effect: 94 CHF per month, 2.8 percent, more if supporting programs increase by 10 percentage points.

Column (7) shows the full model. Results indicate that all aspects matter: program participation, caseworker policy regimes, and PES policies. The program participation effects and the PES policy effects are similar to the models that included only part of all explanatory variables, and so are caseworker policy effects.³³

Whether neglecting regime effects biases estimates of individual treatment effects is an interesting question. Table 4 shows estimates of the individual treatment effects with and without controls for regime effects. Both sets of estimates are similar, as individual treatment effects are identified within the same labor market, i.e. controlling for PES fixed effects as in column (4).

Effects on unemployment exit rate.

Table 5 shows how policy regimes and program participation affect the transition rate from unemployment to regular jobs.³⁴ Columns (1) to (3) show the effects of program participation on the rate of leaving unemployment. Results indicate that both supporting and restricting programs reduce the hazard of leaving unemployment. That supporting programs prolong

unemployment duration is well in line with existing research (Card et al., 2010). Our result on restricting policies goes, at first sight, counter the existing literature on benefit sanctions. But bear in mind that our set of restricting policies encompass both benefit sanctions and participation in workfare programs. When we decompose the effects of these two programs on the unemployment exit rate, we find a positive effect of benefit sanctions on the unemployment exit hazard, and a negative effect of work-fare programs on the exit hazard (see Table 10 in the Appendix). Thus, our results are not contrary to the existing literature.

Column (4) in Table 5 shows policy regime effects at the caseworker level. Results indicate that both, increasing the rate at which job seekers enter supporting and restricting programs increases their rate of leaving unemployment. But the quantitative magnitude differs. Supporting policies increase the rate of leaving unemployment about two thirds as strongly as restricting policies. Column (5) discusses policy regime effects at the PES level. Also for the PES, our results indicate that, both, regimes that use supporting as well as restricting policies often tend to produce a higher exit from unemployment, with supporting regimes playing a quantitatively more important role.

Column (6) introduces both caseworker regimes as well as PES regimes. Interestingly, all levels of policy implementation matter. Supporting policies improve the rate of leaving unemployment more strongly at the PES level than at the caseworker level. Restricting policies are as effective at the caseworker and at the PES level.³⁵

6.2 Explaining the post-unemployment effects: employment vs. earnings

Earlier we have seen that supporting treatments improve earnings of participants, and supporting PES policy regimes also increase earnings after leaving unemployment. These effects might arise because job seekers accept better paid jobs - or because they keep those jobs for a longer time period.

Table 6 shows treatment and regime effects on employment, the proportion of the time we observe after unemployment spent in employment, and earnings while employed,³⁶ average earnings in the months a job seeker is considered as employed. Consider first results for employment. Columns (1) to (3) show treatment effects, caseworker, and PES effects, separately. Column (4) shows results on all dimensions. Treatment effects are positive for supporting policies and negative for restricting policies. Supporting programs therefore enhance job stability whereas restricting policies can induce job seekers to accept less stable employment. Policy regimes also affect job stability. PES offices that use supporting policies more intensely help their clients find jobs that they keep over a longer period whereas PES that build on restricting policies tend to reduce job stability of their clients. Caseworkers also matter but less than the PES. Caseworker use of supporting policies only marginally affects job stability, whereas use of restricting policies reduces job stability, by about half of an equivalent change in the PES' use of restricting policies.

Table 6 also shows results for average earnings of workers. Note that workers are a subset of the population, so these results do not have a causal interpretation. Columns (5)-(7) add treatment and policy regime effects step by step. Column (8) provides results on all dimensions that we consider. Supporting policies increase earnings of the employed by about 50 CHF, or about 1-2 percent of average monthly earnings while employed. Job seekers who had been exposed to restricting policies earn considerably less, 213 CHF per month, or 5-6 percent of average monthly earnings.

Intense use of restricting, and supporting, policies by caseworkers tends to reduce job seeker's earnings while employed. The effect is fairly small and on the margin of statistical significance for the supporting regime, but sizeable and significantly different from zero for the restricting regime. In contrast, PES offices that place a strong emphasis on supporting policies manage to place job seekers in jobs that pay them substantially more. PES who use restricting policies intensely reduce the earnings their clients take home substantially.

Lechner *et al.* (2011) study short- and long-run effects of publicly sponsored training for job seekers in Germany. Initial results all indicate that training prolongs unemployment. But medium-run and long-run effects of these programs are positive, suggesting that the most intensive forms of training can raise employment rates by up to 10 percentage points which are sustained for up to eight years. These effects are consistent with our findings for supporting programs.

An interesting pattern of results emerges. Supporting policies help participants by placing them into more stable employment (at the cost of prolonged unemployment). Restricting policies may (or may not) improve the speed at which job seekers leave unemployment, but damage their post-unemployment prospects via reduced stability of employment and lower earnings while employed. Caseworkers matter strongly for exit from unemployment, but they affect post-unemployment job prospects relatively little. PES are both key to how job seekers leave unemployment and they shape post-unemployment job prospects strongly.³⁷

6.3 Policy interaction effects between carrots and sticks

We have seen that policy regime effects matter, both for caseworkers and especially for PES offices. But so far, we have considered supporting and restricting policies in isolation. Here we study whether combining policy regimes changes their effectiveness.

Table 7 repeats our baseline estimates for policy regime effects on unemployment exit in column (1) and displays results that have interaction terms for policy regimes in column (2). Results in column (2) indicate that both policy interactions are positive and significantly different from zero, and sizeable. These results indicate that having a bit of both, supporting and restricting, policies improves their effect on unemployment exit. Columns (3) and (4) in Table 7 show results for earnings after leaving unemployment. Results in column (4) indicate that the caseworker interaction term is positive and significant, whereas the interaction term for the PES regimes is not significantly different from zero.

These results support the view that restricting and supporting programs are complements. Caseworkers who use a bit of both will have a client pool that leaves unemployment faster, and earns more after leaving unemployment, than caseworkers who specialize in one of the two policies. Complementarity also applies to PES policies, but only as far as unemployment exit is concerned.³⁸ Based on these findings, we conclude that regime effects not only matter in isolation but also via their interplay. To achieve a more comprehensive, quantitative assessment of this trade-off, it is important to consider marginal effects as well as costs and benefits of regime changes. This is done in the next subsection.³⁹

6.4 Policy Experiments

We use the results of Table 7 to perform three policy experiments: we calculate marginal effects to quantify how changes in regime intensities aiming at an increase of unemployment exits (θ_u) or of earnings actually affect the individual outcomes.⁴⁰

Experiment 1 simulates a region that becomes more “active” by increasing the intensity of the carrots and sticks policy regime, from the median level, by adding half a standard deviation. The resulting marginal effects are reported in Table 8. This experiment yields an increase of the job seeker’s unemployment exit hazard rate, by 8.0% due to caseworker regime and 10.2% due to PES regime. Additionally, there is an indirect effect of regime changes, because they affect the amount and composition of the treated; this indirect impact reduces the hazard by 1.5% ⁴¹ Taken together, the total of the marginal effects of regime changes is of the same size as the stick treatment effect on unemployment exit (16.8%, compare to Table 5). The marginal effect of a shift in regimes might, however, also affect job seeker’s post-unemployment earnings. Our results suggest that becoming “active” would affect earnings only very little, increasing them by about 1 percentage point due to caseworkers (whereas PES regime and indirect effect are zero, see Table 8).

The second experiment is to become "supportive", shifting the intensities of the carrot regime up and of the stick regime down (Experiment 2a, Table 8). Becoming "supportive" can have substantial positive effects on job seekers' earnings after they left unemployment. "Supportive" regions raise job seekers' earnings by 4.9%, primarily through the PES regime, with caseworkers adding 0.8% and the indirect effect 0.3% to this earnings gain. Adopting a supportive regime can thus raise earnings by up to 6%. Of course, supportive regimes may affect the time job seekers take to leave unemployment. Indeed, job seekers have 1.9 percent lower unemployment exit hazards, due to the PES supportive regime, and 0.3 percent lower unemployment exit due to caseworkers as well as 0.8 percent reduction due to the indirect effect. Supportive regimes reduce the rate at which people leave unemployment by 3 percent.

A third policy option is to become restrictive, shifting the intensities of the stick regime up and of the carrot regime down (Experiment 2b, Table 8). Our simulations suggest that this restrictive regime has detrimental consequences for post-unemployment earnings. Job seekers would find jobs paying them about 5.0% less due to the PES regime, and a further 1.1 % less due to the caseworker regime; the indirect effect reduces earnings by 0.3%. Restrictive regimes depress earnings in total by about 6.4%, overall. In contrast, being restrictive encourages unemployment exit. Indeed, unemployment exit rates are in total 1.6% higher, mostly due to the PES regime that becomes restrictive.⁴²

Policy experiments also affect the UI budget. We now compare changes in benefit payments due to marginal regime effects on unemployment durations (and benefit cuts because of sanctions) with the incremental cost of adapted treatment intensities. The details on the performed simulations are described in section A.2 of the Appendix.

Regions that become "active", increasing both carrots and sticks, reduce individual unemployment duration by 27 days. Besides this direct impact of the regime changes, their indirect effect (causing a change in the amount and

composition of the treated) prolongs unemployment by 5 days. On net, switching to active regimes lowers unemployment benefit payments by 2284 CHF (Table 9, Experiment 1). Note that the regime effects affect the full sample of unemployed. Active regimes also save on benefit payments due to benefit sanctions, 82 CHF on average. The savings due to additionally imposed sanctions are marginal⁴³. The direct cost of additional supporting treatments caused by the upward shift of the carrot regime amount to 240 CHF due to more intense training, and 77 CHF due to increased job search assistance, or about 317 CHF p.p. in total. The direct cost of strengthening the stick regime results in an increase of 14 CHF because of stricter monitoring, and 170 CHF due to workfare programs, i.e. a total increase of 184 CHF. Thus, in total the UI budget decreases by 1866 CHF or 7.3% of total benefit cost per person by marginally tightening both regimes.

The supportive regime increases the UI budget, because they pay more unemployment benefits, due to longer spells and reduced sanction intensity (Table 9, Experiment 2a). Becoming supportive is less costly in terms of the costs of operating the policy regimes. Supportive regions pay some more to train and assist job seekers, but recoup most of the cost by being less restricting. On net, supportive regions pay 849 CHF more, or about 3.3% of the cost per job seeker.

The restrictive regime does not affect the budget much, reducing it by 237 CHF, or 0.9% (Table 9, Experiment 2b). Shifting some treatment intensity from stick to carrot keeps the operating costs neutral. Moreover, because both sticks and carrots regimes improve unemployment exit to the same extent, there are no first order effects on unemployment benefit payments.

Our simulations illustrate that the policies that reduce the UI budget are not the same as the policies that maintain post-unemployment earnings capacity. The active policy regime reduces the UI budget more strongly than the other policy options. However, the supportive policy regime increases post-unemployment earnings the most.

7 Conclusions

Policy regime effects are quantitatively important. Both caseworkers and PES agencies can strongly increase the exit rate from unemployment by using supporting or restricting policies more intensively. PES agencies are also very important for earnings after leaving unemployment. Supporting policies increase earnings after job seekers leave unemployment, restricting policies reduce them. Quantitatively, restricting regimes are more important for earnings than supporting regimes. Interestingly, caseworkers are much less important for post-unemployment outcomes than for the duration of the unemployment spell.

We also find treatment effects to be important. Both supporting and restricting programs tend to prolong unemployment, restricting programs do so because of the workfare component. Supporting programs then increase earnings after unemployment substantially, whereas restricting programs decrease what job seekers take home.

What do our results imply for labor market policy? First, we document that policy regimes matter. Compared to the actual treatment effects, policy regime effects are small, but they affect a much larger group of job seekers, so they produce sizeable aggregate effects. Evaluations of ALMP that only take treatment effects into account miss a substantial part of the effects triggered by ALMPs. From a methodological point of view, our findings imply that omission of regime effects in studies of treatment effects may lead to violation of unconfoundedness assumptions.

Second, supporting and restricting policies affect unemployment duration in the same way, whereas their effect on earnings is opposite. Both supporting and restricting policy regimes shorten unemployment duration but supporting policy regimes increase earnings whereas restricting policy regimes decrease them. A similar comparison holds for actually attending the programs. Both supporting and restricting programs prolong unemployment, but supporting programs increase pay whereas restricting programs reduce it.

Third, interactions between policy regimes are present and suggest supporting and restricting policies are complements. Agencies that focus on maximizing exit from unemployment would find using both restricting and supporting programs helpful. At the same time, this strategy has a further payoff in terms of increased post-unemployment earnings. Combined use of the two policy instruments appears to dominate specializing in just one of them.

We end this section by highlighting two extensions of our approach. First, one could allow for unobserved heterogeneity of unemployed workers. Yet this requires even larger data sets and leads to an even higher computational burden. Second, regime effects could be further decomposed beyond distinguishing carrots and sticks. The proposed flexible regime estimation strategy can directly be applied to estimating separate regime effects for a refined set of ALMPs.

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A Online Appendix

A.1 Supplementary tables

A.2 Marginal regime effects and simulations for cost-benefit analysis

A.2.1 Marginal regime effects on exit hazards and post-unemployment earnings

In the case of the unemployment exit outcome we calculate the marginal effect – expressed as a percentage change in the exit hazard – as follows:

Define $\tilde{F}_n = med(F_n)$ as the median intensity of policy regime $n \in \{c, s, cs\}$ (c=carrot, s=stick, cs=interaction of both); whereby $\tilde{F}_{cs} = \tilde{F}_c \tilde{F}_s$. Also define $\tilde{F}'_n = med(F_n) + 0.5sd(F_n)$, i.e. the policy intensity shifted by 0.5 standard deviation (+ or –, depending on the experiment); whereby $\tilde{F}'_{cs} = \tilde{F}'_c \tilde{F}'_s$. The direct effect is $\frac{\theta(x, \tilde{F}'_c, \tilde{F}'_s, \tilde{F}'_{cs}) - \theta(x, \tilde{F}_c, \tilde{F}_s, \tilde{F}_{cs})}{\theta(x, \tilde{F}_c, \tilde{F}_s, \tilde{F}_{cs})}$; this reduces to

$exp[\pi_c 0.5sd(F_c) + \pi_s 0.5sd(F_s)] - 1$. The additional interaction effect amounts to $\frac{\theta(x, \tilde{F}'_c, \tilde{F}'_s, \tilde{F}'_{cs}) - \theta(x, \tilde{F}'_c, \tilde{F}_s, \tilde{F}_{cs})}{\theta(x, \tilde{F}_c, \tilde{F}_s, \tilde{F}_{cs})}$. Note that – for simplicity of exposition –

we ignore here the fact that every type of policy regime appears twice, once for caseworkers and once for PES. Thus, we compute the presented effects separately for caseworkers and for PES. On top of the direct effect and the interaction effect of marginally changing F_n , we also compute the indirect effect of such policy changes. This effect arises through the fact that changing the policy intensities F_n leads to a change in the population of job seekers who are treated. To calculate this effect, we proceed in two steps. First, we determine who is additionally non-/affected by the corresponding experiment and, among those, who is predicted to have non-zero observed treatment exposure. The procedure to determine this is described in the steps (2) and (3) in the next subsection A.2.2. Second, we compute the indirect effect by predicting $\theta(x, \tilde{F}'_c, \tilde{F}'_s, \tilde{F}'_{cs})$ for all individuals once with adjusted treatment exposure and once with existing treatment exposure. To compute this effect

as relative change we take the difference of the before-mentioned predictions and divide it by the prediction based on the existing treatment exposure. Note that this indirect effect is caused by policy changes in F_n , but it is purely driven by compositional changes in the treated population which affect the treatment effects predictions.

In the case of post-unemployment earnings the total marginal effect around median policies is

$\pi_c 0.5sd(F_c) + \pi_s 0.5sd(F_s) + \pi_{cs} [\tilde{F}_c 0.5sd(F_s) + \tilde{F}_s 0.5sd(F_c) + 0.25sd(F_c)sd(F_s)]$, with obvious decomposition in direct and interaction effect. This absolute marginal earnings effect is finally standardized by expressing it as a percentage change in non-treated average earnings (3342 CHF). The indirect effect on earnings is computed in the same way as exposed above for the hazards. The results of all these computations are reported in Table 8 in the main text.

A.2.2 Marginal regime effects on unemployment durations and treatment cost

Additional cost and benefits from marginal regime effects arise (primarily) from the resulting changes in unemployment durations, on one hand, and changes in the amount of assigned treatments, on the other hand. Thus, to perform a cost-benefit analysis we need to quantify these changes in a tractable unit and multiply them with corresponding cost/benefit rates per unit. To achieve this, we proceed in several steps.

(1) Predict the effects of a marginal regime change on unemployment durations.

A shift of the policy regimes affects everyone in the sample. Therefore, we first simulate the marginal effect of changing the regimes by 0.5 s.d. on expected unemployment duration for the full sample. Using the estimation results of the interacted model for $\theta_u(t)$ (see Table 7, column (2)), we compute the following individual-level predictions:

$$E[T_u | x, \tilde{F}'_c, \tilde{F}'_s, \tilde{F}'_{cs}] - E[T_u | x, \tilde{F}_c, \tilde{F}_s, \tilde{F}_{cs}]$$

following the (simplified) notation defined in the section above. I.e., we calculate the predicted individual unemployment durations with the policy

regime variables set once to median $\pm 0.5s.d.$ and once to median level (leaving the treatment effects and other x variables unchanged). The difference between the averages of the individual predicted unemployment duration under changed and under median regime represent the marginal regime effects (of the corresponding policy experiment). Since this effect applies to everyone, it is evaluated for the full sample. In addition, we also calculate the indirect effect for each experiment, which arises through the compositional adjustment of who is exposed to treatment due to the marginally changing policy regimes. To do this, we proceed in the same way as described in the last subsection A.2.1, but now we compute the corresponding predictions with adjusted versus existing treatment exposure for T_u .

(2) More (or less) individuals get carrots/sticks due to the marginal regime change: use F_c/F_s rank to determine these additionally (non-)affected people.

Due to the nature of the policy regime definition, as derived in section 4.2, the ranking by F_c and F_s directly allows us to determine in the data who is additionally (not) affected (any more) by the marginal regime shift. Since F_c and F_s represent probabilities to be affected by a certain type of treatment event, it suffices to sort the population according to these intensity measures to determine the group within 0.5 s.d. above (below) the median intensity as the additionally (non-)affected individuals. These groups are determined for each of the four regimes ("carrot" or "stick", by CW or PES).

(3) Predict the treatment durations/incidences which are implied by applying a carrot/stick regime: for additionally (non-)affected individuals.

Our UIR-SSR database contains detailed individual-level information on realized treatment durations or incidences for the observed treated individuals. In particular, we observe and distinguish four treatment types per individual: (1) total duration of training, (2) total duration of job search assistance program participation, (3) total duration of workfare program participation, (4) total number of enforced benefit sanctions and the related number of days of cut benefits. We use these data to predict the duration/incidence of treatment of additionally (non-)affected individuals as

follows. First note that – due to the fact that by far not everyone is observed treated (see Table 3) – we have to deal with large proportions of zeros in the treatment durations/incidences. Therefore, we first regress an indicator of zero treatment outcome on all the control variables (as used in the main estimations) for the population affected by the respective median intensity regime. Then, we predict the probability of a zero treatment outcome for the additionally (non-)affected individuals and rank/sort them according to this probability. Assuming that the proportion of zeros is the same as in the median intensity population, we determine according to this ranking who is supposed to get a non-zero treatment exposure. For those, we predict the outcome duration/incidence of the corresponding treatment type, based on a regression⁴⁴ of the treatment outcome on all controls for the non-zero outcomes in the respective median intensity population. Note that these steps are performed for each of the four types of treatments and for each policy regime ("carrot" or "stick", by CW or PES).

(4) Cost data per treatment type and year: sources and calculation of per-unit figures.

The Swiss State Secretariat of Economic Affairs (SECO) provided us specific cost data per type of treatment and year (from the process data monitoring of the UI). We combine this information with individual-level counts of treatment durations/incidences from the UIR-SSR database to calculate the average treatment cost per unit (for ALMP: per day; for sanctions: per incidence) and type and year. Moreover, we collected the figure of the average daily benefits paid to males by year from official statistics⁴⁵.

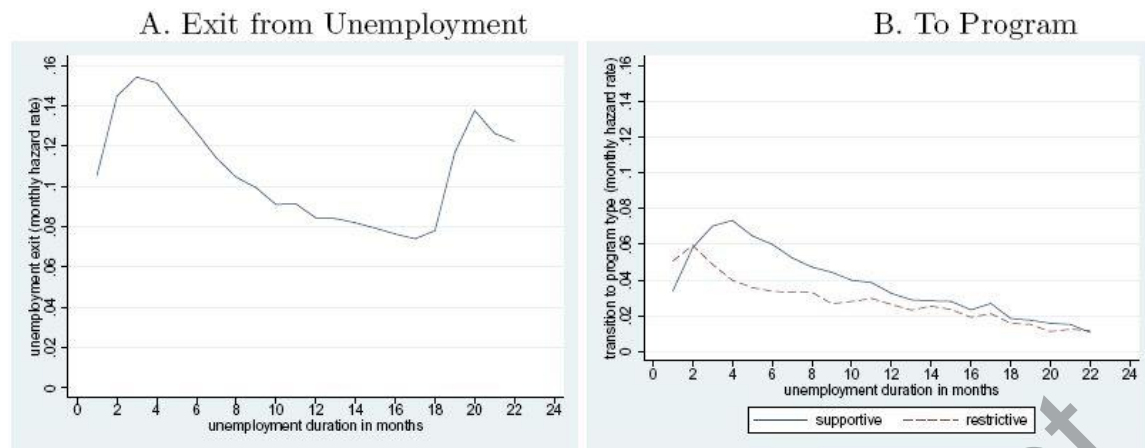
(5) Calculation of total cost: (treatment durations/incidences · per-unit costs), summed over all relevant treatment types.

Finally, we multiply the predicted treatment durations/incidences with the corresponding cost figures. For "carrot" regimes we consider training and job search assistance treatment durations, for "stick" regimes incidences of imposed sanctions and workfare durations. To complement the picture, we also compute the saved days of benefits due to sanctions in the same manner. By summing up these components of additional (less) cost across additionally (non-)affected individuals, we obtain the total marginal cost of the

corresponding regime change. Since the regime change affects everyone, this cost is divided by the full sample population. We report the final figures by treatment type and as a total per regime. This total change in cost is, moreover, expressed as a proportion of the average total benefit cost (i.e., the predicted unemployment duration under median regimes multiplied by daily benefits) of a job seeker.

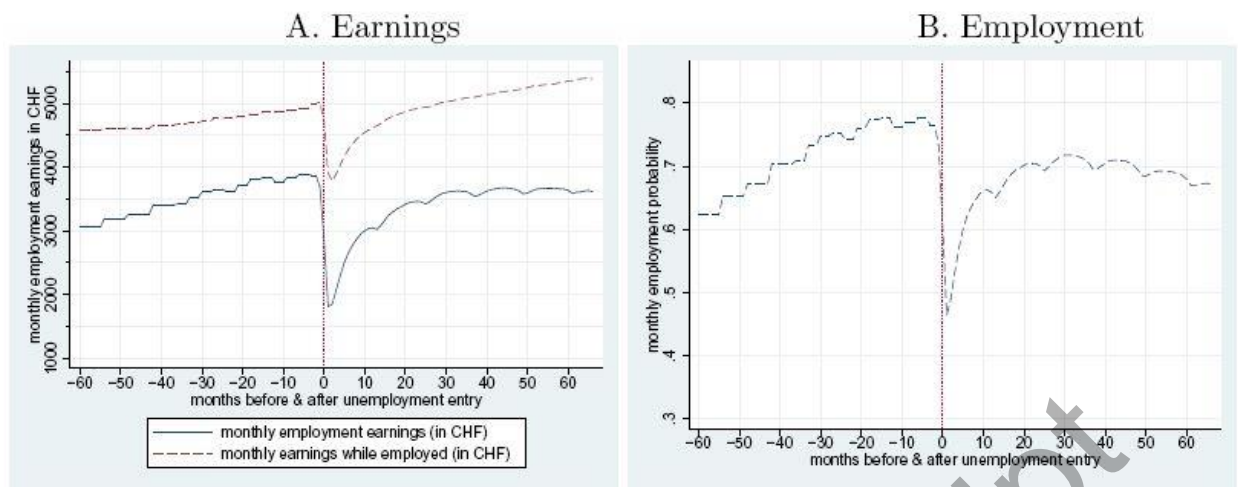
All these steps are performed for each case of the three experiments 1, 2a and 2b, which imply positive or negative changes of the "carrot" and/or "stick" intensities by 0.5 standard deviations, respectively. The results are reported in Table 9 in the main text.

Fig. 1 Transition rates



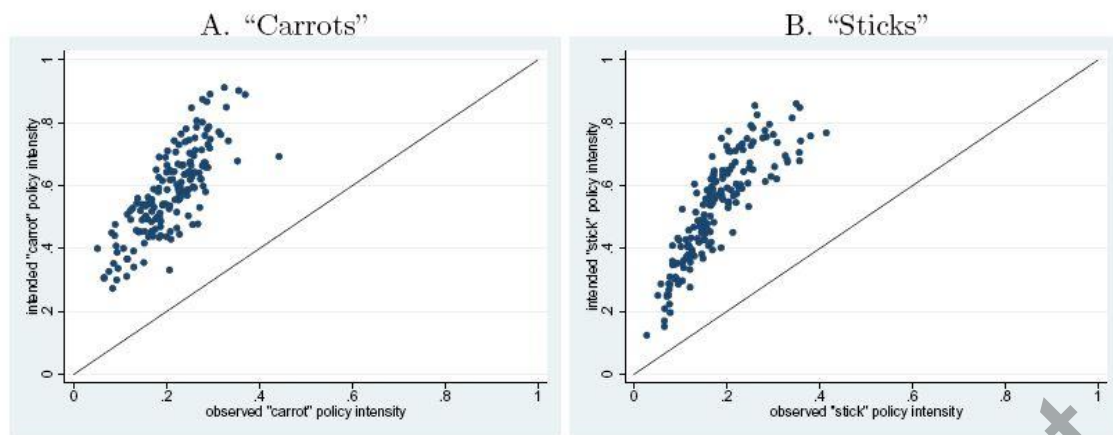
Notes: Graph A shows the empirical transition rate out of unemployment. Graph B shows the empirical transition rate to restricting (sticks) programs, and to supporting (carrots) programs. Restricting programs are benefit sanctions and workfare programs. Supporting programs include job counselling and training programs. *Source:* Swiss UIR-SSR Data.

Fig. 2 Earnings and employment paths



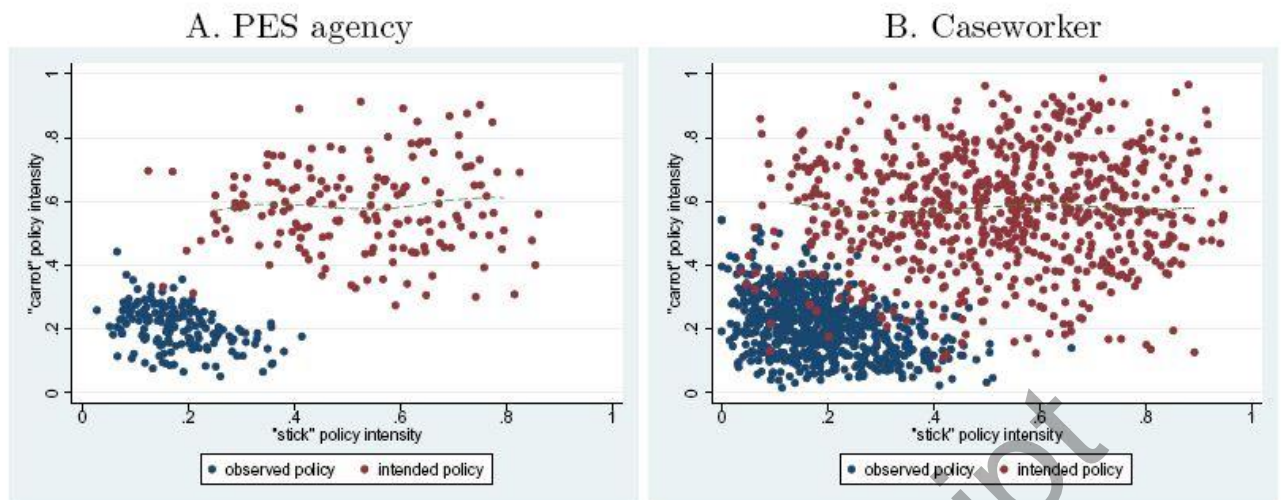
Notes: Graph A shows two earnings measures. “Earnings while employed” (dashed line) represent average earnings among those who are employed during the month. “Earnings” (solid line) measure average earnings, i.e. with zero earnings in case of non-employment. Graph B depicts employment. Earnings and employment paths are relative to the month of entry into unemployment according to the unemployment duration measure in the unemployment register. *Source:* Swiss UIR-SSR Data.

Fig. 3 Observed and intended policy intensities by PES; “carrots”, “sticks”



Notes: This figure shows intended use of programs on the vertical axis vs observed use of programs on the horizontal axis. The solid line represent the 45 degree line. Each dot is a PES agency.

Fig. 4 Observed vs. intended policies by PES and caseworker



Notes: This figure shows observed (dark) and intended (red) intensities of carrots and sticks. Each dot represents a PES agency (left) or a caseworker (right). The dashed line presents the association between the two policies (based on lowess smoother).

Table 1 Descriptive Statistics

		median or mean	sd
Unemployment duration	(median, days)	144	
<i>Realized treatments</i>			
supportive ("carrots")	(incidence)	0.2194	0.414
supportive: duration	(median, days)	97	
restrictive ("sticks")	(incidence)	0.1870	0.390
restrictive: duration	(median, days)	71	
<i>Socio-demographic characteristics (selection)</i>			
marital status	single	0.453	
	married	0.463	
education	compulsory (-9y.)	0.276	
	vocational short (-11y.)	0.094	
	vocational degree (-13y.)	0.504	
	high school (-13y.)	0.028	
	tertiary	0.098	
occupation	blue collar	0.136	
(3 biggest)	construction	0.138	

		media n or mean	sd
	gastronomy, cleaning	0.134	
employability	low	0.145	
	middle	0.718	
age (years)		36.1	11.0
household size		2.16	1.35
not swiss		0.422	
doesn't speak local language		0.394	
Unemployment spells		131 037	

Notes: Sample used in main estimations (men, aged 20-61.5). Mean proportions if no other unit is stated. Realized treatments: incidence=at least on realized treatment of corresponding type (supporting, restricting); duration=duration from unemployment entry to realization of the treatment.

Source: Swiss linked Unemployment Insurance and Social Security Register (UIR-SSR) Data

Table 2 Leeway in the Swiss ALMP System

	Canton to PES	PES to Caseworker
	percent	percent
(1) no guidelines (freedom)	4.08	13.27
(2) rough guidelines	57.14	56.12
(3) detailed guidelines	32.65	29.59
(4) very detailed guidelines	5.10	0.00

Notes: Responses to the question “How detailed are the directions that you receive from your supervising agency (canton)?” for the Canton to PES column, and “How detailed are the directions that you give to your caseworkers?” for the PES to Caseworker column. One person provided no answer to both questions. 98 heads of PES.

Source: Frölich *et al.* (2007) Survey.

Table 3 Observed frequencies of policy usage and intended policy intensities, by PES and by caseworkers. Descriptive statistics

			mean	median	s.d.
<i>observed</i>	<i>PES</i>	"carrot"	0.2155	0.2247	0.0615
	<i>PES</i>	"stick"	0.1853	0.1723	0.0727
	<i>cw.</i>	"carrot"	0.2150	0.2263	0.0647
	<i>cw.</i>	"stick"	0.1848	0.1852	0.0690
<i>intended</i>	<i>PES</i>	"carrot"	0.5843	0.5895	0.1510
	<i>PES</i>	"stick"	0.5292	0.5288	0.1833
	<i>cw.</i>	"carrot"	0.5859	0.5958	0.1707
	<i>cw.</i>	"stick"	0.5315	0.5324	0.1998
<i>Observations</i>			131,037		

Notes: Calculations based on main sample (males aged 20-61.5). *cw.* = caseworker. Observed frequencies are averages per PES or per caseworker. The estimation of the intended policy intensities is described in Subsection 4.2. We distinguish between 168 PES and 717 caseworkers (small caseloads below 100, males and females, are aggregated in a residual caseworker category/dummy variable).

Source: Swiss UIR-SSR Data.

Table 4 The effect of carrots and sticks policies and treatments on monthly earnings (over 3.5 years; men)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
carrot TE	280.5** *		162.3** *	152.7** *	158.4** *	157.6** *	163.8** *
	(19.36)		(21.33)	(21.27)	(21.35)	(21.21)	(21.32)
stick TE		- 368.8** *		- 308.6** *	- 286.2** *	- 284.0** *	- 265.6** *
		(16.30)		(17.96)	(18.19)	(18.04)	(18.45)
sanction TE			- 348.3** *				
			(18.84)				
workfare TE			-70.16**				
			(34.56)				
carrot policy CW					36.03		-13.52
					(71.03)		(70.33)
stick policy CW					- 507.1** *		- 429.1** *
					(79.25)		(74.59)
carrot policy PES						942.6** *	932.4** *
						(137.9)	(136.1)
stick policy PES						- 1,061** *	- 1,014** *

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
						(165.1)	(159.8)
intercept	3,223** *	3,216** *	3,215** *	3,210** *	3,443** *	3,373** *	3,573** *
	(70.79)	(70.79)	(70.79)	(70.79)	(80.41)	(97.55)	(111.8)
obs. (spells)	131,037	131,037	131,037	131,037	131,037	131,037	131,037
R^2	0.380	0.381	0.382	0.382	0.382	0.377	0.377
FE at level	PES	PES	PES	PES	PES	MS	MS

Note: TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5 The effect of carrots and sticks policies and treatments on unemployment exit (hazard rate; men)

	(1)	(2)	(3)	(4)	(5)	(6)
carrot TE	-0.342***		-0.407***	-0.418***	-0.407***	-0.416***
	(0.00819)		(0.00882)	(0.00888)	(0.00883)	(0.00888)
stick TE		-0.00731	-0.171***	-0.185***	-0.171***	-0.184***
		(0.00795)	(0.00855)	(0.00863)	(0.00856)	(0.00863)
carrot policy CW				0.188***		0.165***
				(0.0297)		(0.0300)
stick policy CW				0.274***		0.267***
				(0.0260)		(0.0262)
carrot policy PES					0.363***	0.344***
					(0.0527)	(0.0529)
stick policy PES					0.272***	0.248***
					(0.0405)	(0.0406)
intercept	-4.718***	-4.750***	-4.724***	-4.943***	-5.254***	-5.441***
	(0.0311)	(0.0311)	(0.0311)	(0.0355)	(0.0380)	(0.0411)
obs. (spells)	131037	131037	131037	131037	131037	131037
log-likelihood	-198722	-199705	-198519	-198433	-198685	-198608
FE at level	PES	PES	PES	PES	MS	MS

Note: TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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Table 6 Explaining the post-ue effect: employment propensity (proportion of months employed within observation window) vs. monthly earnings while employed (over 3.5 years; men)

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	<i>effect on employment</i>				<i>effect on earnings while employed</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
carrot TE	0.0347***	0.0350***	0.0353***	0.0355***	37.17*	44.34**	41.68**	49.43**
	(0.00273)	(0.00274)	(0.00273)	(0.00274)	(20.05)	(20.12)	(20.12)	(20.19)
stick TE	-0.0397***	-0.0368***	-0.0361***	-0.0336***	-234.0***	-221.9***	-222.1***	-212.7***
	(0.00271)	(0.00273)	(0.00271)	(0.00274)	(17.53)	(17.63)	(17.58)	(17.84)
carrot policy CW		0.0184**		0.0176*		-114.9*		-166.9***
		(0.00904)		(0.00909)		(63.74)		(64.37)
stick policy CW		-0.0690***		-0.0627***		-239.4***		-173.7***
		(0.00865)		(0.00845)		(68.63)		(66.32)
carrot policy PES			0.0534***	0.0494***			771.6***	787.2***
			(0.0168)	(0.0167)			(120.6)	(120.1)
stick policy PES			-0.127***	-0.120***			-619.3***	-603.3***
			(0.0149)	(0.0145)			(142.5)	(139.3)

	<i>effect on employment</i>				<i>effect on earnings while employed</i>			
intercept	0.718***	0.744***	0.741***	0.763***	3,882***	4,052***	4,018***	4,165***
	(0.0101)	(0.0113)	(0.0127)	(0.0137)	(64.98)	(74.51)	(91.80)	(104.8)
obs. (spells)	131,037	131,037	131,037	131,037	119,033	119,033	119,033	119,033
R^2	0.169	0.170	0.167	0.167	0.431	0.432	0.426	0.426
FE at level	PES	PES	MS	MS	PES	PES	MS	MS

Note: The outcome variable in models (1) to (4) is the proportion of months employed within the post-unemployment observation window (42 months). In models (5) to (8) the outcome variable is average monthly earnings when employed within the same observation period; the number of observations in these models is slightly smaller due to individuals who never generate employment earnings within the observation window. TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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Table 7 Policy interactions between carrots and sticks: marginal substitution/compensation effects when deviating from median policy intensity (ue exit and post-ue earnings; men)

	<i>unemployment exit</i>		<i>post-ue earnings</i>	
	(1)	(2)	(3)	(4)
carrot TE	-0.416***	-0.414***	163.8***	167.0***
	(0.00888)	(0.00889)	(21.32)	(21.33)
stick TE	-0.184***	-0.183***	-265.6***	-264.5***
	(0.00863)	(0.00863)	(18.45)	(18.44)
carrot policy CW	0.165***	0.176***	-13.52	-2.088
	(0.0300)	(0.0300)	(70.33)	(69.56)
stick policy CW	0.267***	0.274***	-429.1***	-417.4***
	(0.0262)	(0.0262)	(74.59)	(75.87)
carrot policy PES	0.344***	0.392***	932.4***	975.1***
	(0.0529)	(0.0533)	(136.1)	(130.9)
stick policy PES	0.248***	0.252***	-1,014***	-1,008***
	(0.0406)	(0.0406)	(159.8)	(160.2)
policy interaction CW		0.307***		673.2**
		(0.105)		(270.0)
policy interaction PES		0.444***		106.8
		(0.132)		(365.8)
intercept	-4.866***	-4.855***	3,350***	3,360***
	(0.0256)	(0.0257)	(62.30)	(62.59)
obs. (spells)	131,037	131,037	131,037	131,037
log-likelihood	-198608	-198585		
R^2			0.377	0.377

	<i>unemployment exit</i>		<i>post-ue earnings</i>	
FE at level	MS	MS	MS	MS

Note: The policy and interaction variables are defined here as deviations from the respective median policy intensity. TE=treatment effect; CW=caseworker; PES=Public Employment Service office; FE=fixed effect; MS=labor market regions (spatial mobility areas). Standard errors in parentheses; *** p <0.01, ** p <0.05, * p <0.1.

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Table 8 Policy experiments: Unemployment and earnings

<i>marginal effects</i>	<i>unemployment exit</i>		<i>post-ue earnings</i>	
	(% change hazard)		(% non-treated earn.)	
	caseworker	PES	casworker	PES
<i>Experiment 1:</i>				
<i>carrots ↑ and sticks ↑ by 0.5 s.d.</i>				
direct effect	0.0433	0.0541	-0.0125	-0.0056
+ interaction effect	0.0370	0.0483	0.0229	0.0032
<i>total</i>	<i>0.0803</i>	<i>0.1024</i>	<i>0.0103</i>	<i>-0.0024</i>
indirect effect	-0.0147		-0.0007	
<i>Experiment 2a:</i>				
<i>carrots ↑ and sticks ↓ by 0.5 s.d.</i>				
direct effect	-0.0123	0.0065	0.0124	0.0497
+ interaction effect	-0.0068	-0.0094	-0.0046	-0.0007
<i>total</i>	<i>-0.0191</i>	<i>-0.0028</i>	<i>0.0079</i>	<i>0.0490</i>
indirect effect	-0.0075		0.0026	
<i>Experiment 2b:</i>				
<i>carrots ↓ and sticks ↑ by 0.5 s.d.</i>				
direct effect	0.0124	-0.0065	-0.0124	-0.0497
+ interaction effect	0.0014	0.0030	0.0011	0.0002
<i>total</i>	<i>0.0139</i>	<i>-0.0035</i>	<i>-0.0113</i>	<i>-0.0495</i>
indirect effect	0.0059		-0.0032	

Notes: Marginal effects are computed based on the results in Table 7. They evaluate shifts of carrot and stick policy regimes from median level by 0.5 standard deviations (see figures in Table 3). See Appendix A.2 for a description how marginal effects are calculated.

Table 9 Policy Experiments: Costs and benefits for the UI budget

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	<i>Experiment 1</i>	<i>Experiment 2a</i>	<i>Experiment 2b</i>
	<i>(C +0.5sd, S +0.5sd)</i>	<i>(C +0.5sd, S -0.5sd)</i>	<i>(C -0.5sd, S +0.5sd)</i>
<i>(A) changes in benefit payments</i>			
...direct effect: in days of UE per person	-26.6	3.0	0.6
...+ indirect effect: in days of UE per person	4.8	3.0	-0.7
...in CHF per person	-2283.8	628.3	-3.6
benefit cuts due to sanctions (CHF p.p.)	-81.9	85.77	-95.6
<i>(B) change in cost of treatments</i>			
<i>carrots</i>			
...for training (CHF p.p.)	239.2	239.2	-242.4
...for job search assistance (CHF p.p.)	76.9	76.9	-78.9
<i>sticks</i>			
...for enforced sanctions (CHF p.p.)	14.1	-14.7	14.1
...for workfare programs (CHF p.p.)	169.3	-166.9	169.3

	<i>Experiment 1</i>	<i>Experiment 2a</i>	<i>Experiment 2b</i>
<i>Total change in cost (A+B; CHF p.p.)</i>	-1866.2	848.5	-237.1
...in % of total benefit cost p.p.	-7.3%	3.3%	-0.9%

Notes: See Appendix A.2 for a description of the simulations and computations of the benefit and cost figures above. 1 CHF = 0.92 EUR = 1.02 USD.

Table 10 The transition of a single-treatment-effect setup to the “carrots and sticks” setup: Decomposition of treatment effects on unemployment exit (hazard rate; men)

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<i>model</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
carrot	<i>yes</i>				<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>
stick		<i>yes</i>	<i>decomp.</i>	<i>decomp.</i>	<i>decomp.</i>	<i>decomp.</i>	<i>yes</i>	<i>yes</i>
censored				<i>yes</i>	<i>yes</i>		<i>yes</i>	
carrot TE	-0.342***				-0.430***	-0.415***	-0.419***	-0.407***
	(0.00819)				(0.00947)	(0.00883)	(0.00946)	(0.00882)
stick TE		-0.0073					-0.0819***	-0.171***
		(0.00795)					(0.00912)	(0.00855)
sanction TE			0.0590***	0.0430***	0.0224**	-0.102***		
			(0.00841)	(0.00973)	(0.00960)	(0.00892)		
workfare TE			-0.340***	-0.481***	-0.546***	-0.535***		
			(0.0188)	(0.0210)	(0.0207)	(0.0192)		
intercept	-4.718***	-4.750***	-4.759***	-4.578***	-4.683***	-4.734***	-4.673***	-4.724***
	(0.0311)	(0.0311)	(0.0311)	(0.0355)	(0.0324)	(0.0311)	(0.0324)	(0.0311)

<i>model</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
obs. (spells)	131037	131037	131037	131037	131037	131037	131037	131037
parameters	296	296	297	297	298	298	297	297
log-likelihood	-198722	-199705	-199479	-168675	-190673	-198251	-191070	-198519
FE at level	PES	PES	PES	PES	PES	PES	PES	PES

Notes: decomp.=decomposition of stick effect into effect of sanction and effect of workfare program; TE=treatment effect; FE=fixed effect; censored= spell is censored at occurrence of first event of the other type (than the reported TE; in model 4, the control group spells are censored at occurrence of a carrot event). Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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Table 11 Second Event, by First Treatment

first event				second event			
	incidence	percent	duration		incidence	percent	duration
"carrot"	28'750	21.94	97	"carrot"	7'554	26.27	196
				"stick"	6'284	21.86	237
				–	14'912	51.87	
"stick"	24'564	18.75	71	"carrot"	4'387	17.86	143
				"stick"	7'267	29.58	130
				–	12'910	52.56	
no policy	77'723	59					
	131'037						

Notes: This table shows the second event for job seekers, grouped by type of first treatment.

Table 12 The effect of carrots and stick policies and treatments on monthly earnings (over 3.5 years;men), Not-controlling for unemployment duration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
carrot TE	-194.4** *		-344.4** *	-344.2** *	-348.0** *	-347.8** *	-350.8** *
	(18.18)		(18.74)	(18.74)	(18.68)	(18.57)	(18.59)
stick TE		-549.1** *		-638.0** *	-630.8** *	-621.1** *	-620.3** *
		(15.69)		(16.22)	(16.25)	(16.22)	(16.32)
workfare TE			-690.5** *				
			(32.83)				
sanction TE			-627.7** *				
			(17.51)				
carrot policy CW					162.9*		157.8*
					(70.35)		(68.57)
stick policy CW					-278.6** *		-55.4
					(73.12)		(61.07)
carrot policy PES						1094.6* **	1014.9* **
						(144.38)	(147.02)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
))
stick policy PES						- 1014.1* **	- 989.8** *
						(170.01)	(162.17)
Constant	3742.3* **	3739.1* **	3785.6* **	3787.3* **	3834.3* **	3821.5* **	3800.1* **
	(65.57)	(65.35)	(65.38)	(65.35)	(78.97)	(106.27)	(111.70)
obs. (spells)	131037	131037	131037	131037	131037	131037	131037
R ²	0.359	0.363	0.365	0.365	0.365	0.360	0.360
FE at level	PES	PES	PES	PES	PES	MS	MS

Notes

¹For lack of better terminology, we also refer to these as supporting and restricting policies.

²Results for women are similar to those we document in this paper. Results are available upon request.

³In particular, if a program expands in its usage then it is plausible that ex ante effects increase in size but it also becomes more likely that firms and non-eligible unemployed workers modify their behavior in response to a sizeable fraction of the workforce being treated. In addition, the changes in the size of ex ante effects may at a macro scale themselves induce further behavioral responses. See e.g. Albrecht *et al.* (2009) for a theoretical and structural analysis and Ferracci *et al.* (2014) and Huber and Steinmayr (2019)

for evaluation frameworks with indirect effects of policies via aggregate labor market outcomes.

⁴ Somewhat related studies consider effects of warnings or notifications of the likelihood of future individual treatments (see Lalive *et al.* (2005) for a “sticks” policy, and Crépon *et al.* (2018) for a “carrots” policy).

⁵ Pavoni *et al.* (2013) discuss the optimal combination of work-first and job-search-first programs in a theoretical setting where skills depreciate over the course of the unemployment spell. Wunsch (2009) adapts the Pavoni-Violante framework to study optimal job search assistance in West Germany.

⁶ Recent progress on mediation analysis in duration settings include VanderWeele (2011). Quantification of direct vs. indirect effects on hazard rates requires a number of assumptions; see e.g. Lange and Hansen (2011) and Kaufman *et al.* (2004). However, regime effects are not mediators of a treatment since they affect, and change, the behavior of potentially all job seekers in a population.

⁷ Below we also discuss additional existing literature with Swiss data in some more detail.

⁸ In our observation window, 1 CHF = 0.96 Euro on average.

⁹ A suitable job has to meet four criteria: (i) the travel time from home to job must not exceed two hours, (ii) the new job contract can not specify longer hours of availability than are actually paid, (iii) the new job must not be in a firm which lays off and re-hires for lower wages, and (iv) the new job must pay at least 68% of previous monthly earnings. Potential job offers are supplied by the public vacancy information system of the PES, from private temporary help firms or from the job seeker’s own pool of potential jobs. Setting the minimum number of job applications is largely at the discretion of the caseworker at the PES.

¹⁰Workers who anticipate losing their job are eligible for training until they start receiving benefits.

¹¹Empirically, subsidized employment rarely starts before the carrot and stick interventions start. Fewer than 3% of treated job seekers in our sample started subsidized employment before the treatment we analyze.

¹²E.g. some areas tested practices where caseworker assignment switches after 6 or 9 months; but this is a minor quantity. Other reasons for occasional assignment changes are that caseworkers leave the PES to look for another job or when they are sick.

¹³Results for women are qualitatively similar to those for men; regime effects are somewhat less strong for women than for men.

¹⁴Some job seekers receive a second treatment during their spell. We focus on the first treatment primarily because it avoids a list of complications that we discuss in Section 4. Note that the first treatment type is somewhat informative on the treatment history. For instance, of those job seekers who enter a supporting program first, 52% experience no second treatment and 22% enter a supporting program for a second time. Similarly, of those job seekers who first experience a “stick” event, 53% experience no further event and 30% experience a second “stick” event. Table 11 in the Appendix displays patterns of first and second treatment. The probability of a second treatment does not depend strongly on the nature of the first treatment, i.e. probability of first treatment exposure is similar to second treatment exposure for those with treatment exposure.

¹⁵The increase in mean earnings primarily reflects eligibility conditions for unemployment benefits which state that job seekers need to have been working in the two years prior to entering unemployment.

¹⁶Changes of more than 20 percentiles (in absolute value) occur for less than 20-40% of the set of PES.

¹⁷ Only 7.6% of the cases are affected by a switch in the CW/PES relation. Moreover, most of these are due to PES reorganizations involving a change in the catchment area or an expansion or merely a change of name. In such cases it is far from clear that the PES environment actually changed for the caseworker. We carried out sensitivity analyses in which we only use such subsets of the data. The signs of parameter estimates are similar to those obtained with the full data set but the reduced sample size results in high standard errors. Below we also discuss estimates of specifications in which the PES parameters are discarded altogether.

¹⁸ We discuss the role of unobserved covariates below. Note that even in their absence, some regularity assumptions need to be satisfied; for example, it is not allowed that one type of treatment can only occur after the other type.

¹⁹ We could extend the competing-risks setting by including observations of the occurrence of a second treatment if that is of a different type than the first. However, this would simultaneously necessitate the estimation of the causal effect of the first treatment at durations in-between the first and second treatment. Clearly, this means a loss of all the computational advantages of our approach. Moreover, it means that we would need to address the occurrence of consecutive treatments of the same type as well, and the estimated intended-policy indicators would be sensitive to the assumptions about chain reactions between treatments and treatment effects as well as the contents of the second treatment.

²⁰ The regime definition assumes that the probability of entering a treatment by the end of a two year period, or 730 days, matters. Setting the regime period to one year, or 365 days, does not change the results, as the estimated probabilities over the two year period (as in the baseline) and the one year period are highly correlated. Estimates based on the one year horizon are almost identical to those based on the two year horizon.

²¹ Clearly, individuals have an incentive to stay unemployed in order to benefit from the treatment, e.g. by rejecting job offers and reducing search effort.

However, such strategic ex ante effects on the outcomes are part of the policy regime effects that we are after in the analysis of the outcomes of interest.

The same applies to scenarios in which PES and CW with intensive supporting regimes invest more effort in getting to know the job seeker. Such a more effective service may enhance the job seeker's search efficiency (how and where to search) and increase job offer arrival rates before the actual job search assistance program participation takes place. Finally, we expect that restricting (sticks) policies have regime effects that affect treated and non-treated. Interestingly, [Lalive *et al.* \(2005\)](#) and [Arni *et al.* \(2013\)](#), who study the effects of unemployment benefit sanctions in Switzerland, document that non-sanctioned job seekers leave unemployment more quickly in PES that use sanctions more often.

²²This setting is inspired by the job search model with monitoring and sanctions in [Abbring *et al.* \(2005\)](#).

²³Notice that in the outcome equation for $\theta_u(t)$ (or, equivalently, for t_u) for, say, individual i , the F_c and F_s terms are estimated in a first stage from which individual i is not excluded. Formally, those estimates depend not only on the observations of the actual treatment statuses and the actual realizations of t_u of other clients with the same CW or PES, but also on the corresponding observations for individual i himself. However, with the sample sizes we have on numbers of clients per CW and PES this problem is of negligible order, and a more sophisticated procedure would be computationally very challenging. Simulations suggest that in our setting the estimates for the parameters of the outcome equations are not affected by this.

²⁴In total, 5.8% of the unemployment spells are right-censored. One reason for the low censoring rate is that we continue to follow everyone after UI entitlement exhaustion by using social security data. We right-censor unemployment durations exceeding 730 days. Some of these may be due to coding errors in the transition date out of unemployment. For the censored observations we use the censored duration in the above equation and we use actual earnings on the left-hand side.

²⁵At the individual level, the estimated regime indicators depend in a non-linear way on individual characteristics that also directly enter the main outcome equations. To investigate how sensitive the results are with respect to the non-linearity embedded in the first stage of the estimation procedure, we have estimated ad-hoc linear specifications of the first-stage equations to obtain alternative estimated regime effects and we used the linear predictions in the second stage. Keeping in mind that such a linear approach is difficult to implement and to justify in a dynamic framework with multiple treatment types, we find that the resulting sanction effect estimates differ somewhat from those based on the baseline specification but the estimated carrot effects are similar (details are available upon request).

²⁶The full list of covariates features: age (9 categories), marital status (3 cat.), highest educational attainment (7), function in last job (5), occupation in last job (16), nationality group (8), knowledge of the regional language (and its interactions with low-level and no qualifications), knowledge in a first and a second foreign language (2 dummies), unemployment spell in past 3 years (dummy), employability scale (5 cat.), residency status (5), potential benefit duration (7), replacement ratio (7), household size (6), disability insurance (DI) application (dummy), partial DI (dummy), eligibility only for ALMPs (dummy), month of inflow into UI (12), log of elapsed unemployment duration (up to 6th polynomial; only in post-unemployment models), MS region fixed effects (105), history of past earnings over 5 years: de-meaned monthly earnings, averages over intervals (months 1, 2-3, 4-6, 7-9, 10-12, 13-15, 16-18, 19-21, 22-24, 25-27, 28-30, 31-33, 34-36, 37-42, 43-48, 49-54, 55-60 before unemployment entry).

²⁷Arni *et al.* (2013) study sanction effects using Swiss data and find that modelling selection due to unobservables becomes unnecessary for the unemployment duration analysis once one conditions on pre-unemployment earnings and employment histories. We have assessed regime effects for program participants and non-participants. We find regime effects are similar and not significantly different for both types of job seekers in all respects, except for caseworker regime effects for carrots. For the latter the statistical

difference is weakly significant (when allowing for a treatment effect). This approximate test suggests that observed characteristics (incl. treatments) capture selection into three regimes well, but less so selection into caseworker carrot regimes.

²⁸Other reasons for assignment were employability and age but these were mentioned by fewer than 10 % of all caseworkers.

²⁹We proceed as follows. The seven types of job seekers are: job seeker who enters unemployment after an apprenticeship, job seeker with good prospects, job seeker with bad prospects, qualified Swiss, un-qualified Swiss, qualified immigrant, un-qualified immigrant. Caseworkers indicate for each job seeker profile whether they think restricting and supporting programs are important. We aggregate the number of times a caseworker finds a program is important and end up with a number that ranges from zero to seven. Seven indicates that a caseworker would use the program regardless of the type of job seeker he or she is serving. Zero indicates the caseworker would never use the program.

³⁰One may consider using the caseworker specific use of a treatment as an instrument for treatment itself, as in e.g. *Markussen et al. (2012)*. This approach fails in our setting, since caseworkers and job seekers entertain a long-term relationship that reveals information on the caseworker regime to the job seeker before any treatment. Indeed, it is one of our aims to investigate whether job seekers react to this information. Adopting the caseworker candidate instrument, we would have to assume that caseworker regime effects do not exist. For this reason we do not present analyses excluding actual treatment effects since such an analysis merely averages policy regime effects and actual treatment effects.

³¹Results are similar at the caseworker level; those are available upon request.

³²The full sample population is observed for 42 months after unemployment exit. Note that the earnings outcome used here contains zeros for months and

individuals without employment. These zero observations are included in order to generate a comprehensive post-unemployment earnings measure that covers the intensive margin (earnings while employed) and the extensive margin (employment or not). The measure will be decomposed along these margins in Table 6.

³³Our estimates assume that there is a linear relationship between outcomes and regimes. We have estimated alternative models that allow for a flexible functional form between outcomes and regimes. These models suggest a monotone relationship, and allowing for more flexibility does not improve the model fit at all. The linear model appears to provide an adequate description of the data.

³⁴We also analysed non-employment duration. Results are similar for treatment effects and carrot regime effects. Stick regime effects for non-employment duration are negative, while they are positive in the baseline analysis. This is consistent with our mixed evidence on stick regime effects on leaving the register, and on post-unemployment earnings.

³⁵Our baseline earnings analyses, in Table 4, control for realized duration of unemployment showing the direct effect of regimes on earnings, but possibly miss an indirect effect of regimes on earnings through changes in unemployment duration. We have estimated models that omit unemployment duration as a regressor. Regime effects are not strongly affected by controls for unemployment duration (CW regime effects increase somewhat but remain small in quantitative terms). Treatment effects can not be identified since treatments are allocated throughout the unemployment spell introducing a mechanical link between treatment status and unemployment duration.

³⁶These only concern the individuals observed to be employed after exit out of unemployment and hence exclude the small percentage of those with right-censored unemployment durations.

³⁷The difference between PES and caseworker effects on post-unemployment outcomes can be understood in two ways. First, recall that the

two caseworker regime indicators display a substantially larger variance than the corresponding PES regime indicators. This may signify that the feasible range of PES regimes is smaller than that of caseworker regimes. In such a case a larger PES coefficient does not necessarily mean that caseworker effects are quantitatively less relevant, as any regime effect is the product of a coefficient and a change in the value of the corresponding indicator.

Secondly, for employment effects the PES regime may be more relevant because total employment may include subsequent unemployment after job loss. If an employed individual expects an upcoming job loss then he may already contact the employment agency and register as soon-to-be unemployed. Such an individual is not necessarily intensively exposed to caseworkers, but one would expect that PES regime plays a role, especially since the individual has prior experience with the same PES regime. In contrast, the caseworker after subsequent reentry into unemployment is more likely to differ from the one in the spell of interest.

³⁸ Complementarity can be genuine or due to serial program attendance. Job seekers might be exposed both to carrot-type and to stick-type treatments, so increasing the intensity of the stick regime will affect outcomes in regions with high-intensity carrot regimes and vice versa.

³⁹ Our specification assumes that regime effects apply to treated and non-treated job seekers alike. If the effect is just an ex ante effect for the first treatments then this is maybe too restrictive. However, recall that policy regimes are potentially more general than just ex ante effects. The policy regime does not become irrelevant after the first treatment, and it is an open question whether the intensity of the involvement of CW and PES increases over time or not. We have estimated models that allow for interaction effects. It turns out that we do not find that regime effects become less relevant after the first treatments.

⁴⁰ Dehejia (2005) discusses a related but different issue, namely what are the gains from asking the caseworker to decide on ALMP. He finds that a flexible assignment rule may provide large gains compared to more rigid rules.

⁴¹This is due to the fact that the carrot and stick treatment effects are dominated by the so-called “lock-in” effect, see Table 5. The lock-in effect, the negative impact on the hazard, is larger for carrot treatments than for stick treatments.

⁴²Restrictive policies promote unemployment exit only little. Recall that both supporting and restricting policies promote unemployment exit, and the effects are similar (Table 7). The restrictive experiment increases restricting programmes and reduces supporting programs generating, on net, small, or no, effects on unemployment exit.

⁴³The average total duration of sanctions imposed on the additionally affected people (due to the strengthened stick regime) is about 14 work days. This amounts to 0.36 work days of additional benefit cut per person of the full sample.

⁴⁴We use OLS regression here. We tested, as an alternative, to apply poisson regression. The “goodness of fit” (as measured by the absolute distance between the prediction and the realization in the median intensity samples) was, however, not better for the latter.

⁴⁵Figures for the years 2000 to 2005. Source: Staatssekretariat für Wirtschaft SECO (2007): Arbeitslosigkeit in der Schweiz 2006, Bern, p.31.